

LETTERS TO THE EDITOR.

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Ornithology of the Sandwich Isles.

IT is easy to make assertions which, however improbable, it is not easy to disprove. I would therefore invite Mr. Albert F. Calvert to furnish documentary evidence of those he has advanced (*supra*, p. 485). They do not indeed materially affect what I had said; yet, for the sake of accuracy, it might be as well to know on what foundation they rest. Those who are interested in the growth of ideas will be pleased to find that Sir Joseph Banks was so far in advance of his time as, on his return from his voyage with Captain Cook, which ended in 1771, to have "several cases of birds carefully mounted and arranged according to the localities in which they were collected"; and, among them, a "group of land birds from Owhyhee"—an island which Cook did not discover until 1778—or seven years later. As these assertions alone concern the subject on which I wrote, I refrain at present from offering any remarks on the others; but your correspondent seems to have been the victim of a delusion or something worse.

ALFRED NEWTON.

I OBSERVE in NATURE (p. 485) a letter from Mr. Albert F. Calvert, in which he states that certain cases of birds, which were collected by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook, "were in the custody of the Linnean Society of London until 1863, when they formed part of their natural history sale."

This is not the fact. The birds belonging to Sir Joseph Banks were never in the possession of this Society. It is true that the Society at one time possessed the insects and shells which formed part of the Banksian collections, and these in 1863 were presented, not sold, to the Trustees of the British Museum. Had there been any birds, they would doubtless also have been presented at the same time.

Where, then, did the cases, which Mr. Albert F. Calvert says are still "carefully preserved in the museum of his ancestor Mr. John Calvert," come from? Certainly not from the Linnean Society. In view of Prof. Newton's valuable communication to NATURE (March 17, pp. 465-69), it is of importance that this inquiry should be answered, and a list furnished of the species contained in these cases, the reported existence of which will agreeably surprise ornithologists.

As Mr. A. F. Calvert has disclosed a source of information likely to be useful, perhaps he may be able to answer another question.

In 1860 the late Mr. J. D. Salmon, a well-known oologist, bequeathed to the Linnean Society a valuable collection of birds' eggs. This collection was known to contain the eggs of many British birds which were then becoming scarce, and have since become still rarer, if not quite extinct as breeding species; such, for example, as the golden eagle, osprey, kite, buzzard, honey buzzard, raven, chough, dotterel; and amongst some of the rarer species not found breeding in Great Britain, the spotted eagle, gerfalcon, black kite, rough-legged buzzard, Lapp owl, &c., and, above all, an egg of the great auk (*Alca impennis*), the value of which alone would almost equal that of all the other eggs in the collection.

I am informed that on the death of Mr. Salmon this collection was intrusted by his executors, for the purpose of being catalogued, to someone known to Mr. A. F. Calvert's ancestor; and by some accident, when it came to be handed over to the Linnean Society, the egg of the great auk was (some time afterwards) found to be missing, as also the eggs of certain species above mentioned, with several others that might be named.

Possibly they may have been removed at the time for safe custody, and were forgotten to be returned when the collection was deposited in its present resting-place.

Perhaps Mr. A. F. Calvert will say whether by chance these eggs (like the Banksian birds) have been "carefully preserved in the museum of his ancestor." If so, I presume that, on proof of the bequest of the collection to the Linnean Society, and proper identification of the eggs by their numbers, initials, or other

marks, Mr. A. F. Calvert would be willing (on behalf of his ancestor) to restore them to the cabinet from which they have been so long missing.

J. E. HARTING.

Linnean Society, Burlington House, March 28.

Poincaré's Thermodynamics.

IT is clear that I was justified in attributing the gist of M. Poincaré's first letter to his not having sufficiently read my notice of his book. He has not even yet fully apprehended the bearings of that notice, as a few words will show. Far from being unable to uphold any one of my critical remarks, as M. Poincaré is pleased to hint may be the case, I reassert every one of them, and could easily add to their number.

Let us begin with the particular item of my criticism which M. Poincaré persists in regarding as the most important. My words were:—"in his assumed capacity [of pure analyst] he quite naturally looks with indifference, if not with absolute contempt, on the work of the lowly experimenter." As an illustration of this I instanced M. Poincaré's ignorance of the thermo-electric researches of Sir W. Thomson, Magnus, &c. Then I quoted (in full) two of his remarks on the "Thomson effect." In the first of these he used the very peculiar phrase

"Sir W. Thomson *admet* qu'il existe une force &c."—and in the second he said

"si l'effet Thomson a pu être mis en évidence par l'expérience, on n'a pu jusqu'ici constater l'existence des forces électromotrices qui lui donnent naissance."

To these he has, in his recent letters, added other like statements. Now, as I understand the matter, Lord Kelvin *proved* (which, as I take it, means a good deal more than might be implied by "constater") the existence of the electromotive force which depends on the so-called "Thomson effect" (giving also thereby the means of measuring its amount) by showing that the Peltier electromotive force does not in general fully account for the observed current in a thermo-electric circuit, and may even be directly opposed to it; while no other source of electromotive force can exist save the gradation of temperature in one or both of the metals. He then proceeded, by experiment, to measure the amount of the "Thomson effect" for unit current in various metals, unequally heated. When the passages above quoted from M. Poincaré's work are compared with the facts just stated, my comments on them will be seen to be fully justified.

It is necessary to add that I made no reference whatever to M. Poincaré's distinctions between "true" and "apparent" electromotive force:—simply because I regard these, along with many other celebrated terms such as "disgregation" &c., as mere empty names employed to conceal our present ignorance.

As to the three chief objections I made to the work of M. Poincaré, every one (author, critic, or onlooker) is entitled to form and express his opinion. I need not restate mine, though I continue to adhere to every word of it:—but I may make the following additional remarks on these objections severally.

1. What sort of title to completeness can be claimed for a Treatise on Thermodynamics, in which no mention is made of the grand principle of Dissipation of Energy; nor of Thermodynamic Motivity, "that possession the waste of which is called Dissipation"?

2. With regard to the measurement of Absolute Temperature, what I *did* say was that the experiments of Joule and Thomson, which justified them in basing it on Carnot's Function, were not mentioned by M. Poincaré *in this connection*. The omission by M. Poincaré of the italicized words makes an absolutely vital change in the meaning of my statement; and enables him to make what, (at first sight only), appears to be an answer to it.

3. As regards the foundation of the Second Law, it is unfortunately clear that M. Poincaré and I must continue to differ:—so that further discussion of this point would be unprofitable. For I presume that M. Poincaré has not formed his opinion without careful study of all that Clerk-Maxwell said on the point:—so that even a perusal of Lord Kelvin's latest paper (*Fortnightly Review*, March 1892) is not likely to induce him to change it.

P. G. T.

26/3/92.

M. Poincaré and Maxwell.

In his recent treatise on "Électricité et Optique," M. Poincaré professes to give a description of Maxwell's theories of electro-

magnetic actions. M. Poincaré appears to think that Mossotti's theory is consistent with and differs but little from Maxwell's. On this Maxwell says (§62):—"The theory of direct action at a distance is mathematically identical with that of action by means of a medium . . . provided suitable hypotheses be introduced when any difficulty occurs. Thus Mossotti has deduced the mathematical theory of dielectrics from the ordinary theory of attraction." Maxwell anyway repudiated Mossotti's theory. M. Poincaré introduces a "fluide inducteur" as the name of a thing displaced in the dielectric, when what Maxwell calls electric displacement occurs. This is all very well. It is anyway not inconsistent with Maxwell, even though Maxwell says distinctly that he does not know what the change of structure is like which he calls electric displacement. It might be a bending or twisting or lots of things, but M. Poincaré is partially justified in fixing the idea thus. He calls this "fluide inducteur" elastic, though at the same time he calls it incompressible. It is not quite clear what "fluide" means here. M. Poincaré certainly observes that the elasticity of the "fluide inducteur" is quite different from that of material bodies, and in fact acknowledges that it is such as can hardly be fairly attributed to an incompressible fluid. Indeed, how can an incompressible fluid be elastic at all? There must be *something* besides the fluid; there must be some structure fixed in space which offers an elastic reaction to the fluid when driven past it, or else there must be the two liquids he objects to that are driven past one another. It is hardly a fair representation to talk of an *elastic incompressible fluid*, and then to invent difficulties, when the phenomena could not confessedly be represented by any such thing, but only by a fluid with some other mechanism superadded.

M. Poincaré's statement, "La méthode précédente n'est pas la seule que l'on puisse employer pour déduire de la théorie de Maxwell les lois de la distribution électrique," coupled with his further statement of "une autre méthode . . . sans supposer l'existence de ce fluide," seems at variance with his implication that this elastic incompressible fluid is part of or involved in Maxwell's theory.

This leads to the question of how far Mossotti's theory can fairly be considered as a substitution for or as a development of Maxwell's. It does not in any real sense get over action at a distance. There are the horrid old electrical charges acting upon one another across a space full of some non-conducting medium. This is practically no advance as far as a theory of electrical action is concerned. It is an advance no doubt as far as the behaviour of the medium is concerned, inasmuch as it enables a time propagation through space to occur; but as a theory of electric action it is a distinctly retrograde step on Maxwell's scientific position that he did not know what was the structure of the ether.

M. Poincaré proceeds to criticize Faraday's theory of the stresses in the dielectric, which he attributes to Maxwell. He begins by suggesting that the forces should have been explicable by the elasticity of the inductive fluid, in the same way as mechanical forces are due to the elasticity of matter. He has in this quite forgotten that what he calls the elasticity of this fluid, is not a bit like the elasticity of any matter, and would require either a second fluid, which he rejects, or some structure other than the fluid, to explain its properties. Granting such an additional structure, then the elastic energy of the medium, fluid and structure combined, *does* exactly explain the motions of conductors. Nobody has explained exactly *how* conductors differ from non-conducting space in structure, and can or do move, and this is not a bit clearer on Mossotti's hypothesis than on any other, not even when the non-conducting diaphragms are made infinitely thin. Maxwell long ago pointed out that no linear system of stress could leave a medium in equilibrium and move bodies immersed in it; and yet M. Poincaré criticizes Faraday's system because it is not linear; and this after remarking himself that the elasticity postulated already was not a bit like that of matter. All that is necessary is some assumption as to the connection between the conducting matter and the dielectric, for the "fluide inducteur" by hypothesis has elastic properties that make it the seat of the right amount of potential energy; and all that can possibly be necessary is to connect the matter with it in such a way that the energy of the medium lost when the conductor moves is given up to the conductor. M. Poincaré has again omitted to remember that the peculiar elasticity of the "fluide inducteur" necessitates some structure with which it is connected, and the Faraday stress may be in this structure, and due to its con-

nection with the "fluide inducteur," and not at all due to another fluid with peculiar properties. If the stresses are due to the connections of the "fluide inducteur" there is no great difficulty in supposing them proportional to the squares of the displacements of the "fluide inducteur," just as the increased tension of a stretched horizontal string due to a small weight at its centre is proportional to the square of this weight. In fact, a suggested model working upon this sort of principle has been published as illustrating this very point, and Dr. Lodge's model ethers, in the first part of his "Modern Views," are all of this kind.

He creates this by assuming that the energy of the medium is all due to the work done by these mechanical stresses deforming it. This is a most gratuitous assumption. Take the case of the stretched string with the weight on it. The increased energy of the system is not due only to the work done by the *increased* tension. At last he confesses, however, that if the energy in the dielectric be kinetic and not potential these difficulties would disappear. "Mais on ne peut encore adopter cette interprétation de la pensée de Maxwell sans se heurter à de grandes difficultés." And why? Merely because Maxwell afterwards calls the electric energy potential while he calls the magnetic energy kinetic. Has M. Poincaré forgotten that potential energy may in any case be the kinetic energy of an associated system? or can he not imagine two modes of motion of the same medium? Anyway, if the potential energy may be the kinetic energy of an associated system, and if M. Poincaré's difficulties are inapplicable to a kinetic explanation of the phenomena, it seems impossible but that they are really inapplicable to a potential system if this system be judiciously devised. It is just here that M. Poincaré fails. He revels in elastic fluids, and yet he continually harps upon the same difficulty—namely, "How can an incompressible liquid be elastic at all?"—and instead of once for all solving this by acknowledging that there must be some structure, he reverts to it as if it were a new difficulty whenever he comes across its consequences.

As a mere mathematical work the book is admirable and clear, if a little prolix. GEO. FRAS. FITZGERALD.

Trinity College, Dublin, March 24.

Prof. Burnside's Paper on the Partition of Energy, R.S.E., July 1887.

IN his criticism on a paper of mine on the partition of energy in a set of non-homogeneous spheres (NATURE, March 31, p. 512), Mr. Watson says that the conclusions are vitiated owing to my having omitted to introduce the frequency factor of collisions before proceeding to take the averages. This is not exactly accurate, since a frequency factor is introduced, viz. the relative speed of the centres of inertia of the impinging spheres parallel to the line of impact.

In the spring of 1888 Prof. Boltzmann published a criticism of the same paper in the *Sitzungsberichte* of the Vienna Academy, in which he contended that the correct frequency factor should be the relative speed of the points of impact of the spheres parallel to the line of impact; and in which he showed that the result of averaging with this frequency factor is to make the mean rotational energy equal to the mean energy of translation. Had I been entirely satisfied at the time of the cogency of Prof. Boltzmann's reasoning, I should, of course, have published a short note calling attention to the correction he proposed to make; and I regret now that this was not done, as it would have prevented the waste of a certain amount of valuable time and trouble.

W. BURNSIDE.

Royal Naval College, Greenwich, April 1.

DR. WATSON has shown in his letter to NATURE of March 31 (p. 512) how the general methods of Maxwell and Boltzmann may be applied to the particular problem discussed by Prof. Burnside. He has also pointed out an error in Burnside's reasoning—namely, the non-introduction of the factor $u - U + c\omega$, whereby Burnside's conclusions at variance with the Maxwell-Boltzmann law of partition of energy are vitiated.

You may, perhaps, allow me space to point out a little more precisely in what, as appears to me, the error consists. Burnside has to find the average value of the expression—

$$(u - U + c\omega) \{2\omega - c(K + k)(u - U)\}$$

(see p. 503). Now, we may take averages in two ways: